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XXXIX. *Some Experiments on Putrefaction; by* F. L. F. Crell, *M. D. and Profeſſor of Chemiſtry at* Brunſwick.

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THE celebrated lord Bacon [a] has, without doubt, ſhewn a very great ſagacity, in pointing out to poſterity, putrefaction, as a ſubject, worthy of making further inquiries into; and certainly, as there happen daily ſo many changes, not only in the inanimate, but alſo in the animate world, carried on by its means; the knowledge of every thing relating to it muſt clear up a great many points in natural philoſophy, not thoroughly underſtood before. But theſe inquiries ought to be ſtill of more conſequence to mankind, as health depends greatly upon keeping in due bounds putrefaction, which the body naturally tends to. For theſe reaſons, Sir John Pringle deſerves, beſides his other eminent merits, very great praiſes, on his having made many experiments on this ſubject; and medicine is indebted to him for conſiderable improvements reſulting from them. He has beſides opened

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[a] Nat. Hiſt. Cent. IV.

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the way to many other gentlemen, among whom excell Dr. Gaber, and Dr. M'Bride, whoſe numerous experiments ſhew the ingenuity, and ſagacity, they are poſſeſſed of: but the ſubject is not yet exhauſted, nor will it be very eaſily. I have made ſome experiments relating to it; and ſhould be very glad, if they threw a new light on ſome points of the greateſt importance to medicine.

Dr. Gaber has proved, by his experiments, the preſence of a volatile alcali produced by putrefaction; but as he did not diſcover by the ſame proceedings [*b*] any in its beginning or end, though there was a very putrid ſmell, he denies its exiſtence in theſe ſtates, and concludes, that this volatile alcali is not a neceſſary product of putrefaction [*c*]. This doctrine ſeemed to me not quite conformable to the phænomena: for, as all ſmell, as much as we know at leaſt till now, depends on a ſaline matter, joined with a phlogiſton, and the ſaline matter producing the putrid ſtench, was not very likely an acid; I ſuppoſed it to be a volatile alkali, which, involved in phlogiſtic matter, might fly off, before the alkali was developed. I wanted to know by experiment, if I was right; for this purpoſe, I put, the 19th of June (the thermometer being 58° of Fahrenheit, and continuing between 58° and 62° all the time I obſerved), in a pretty large receiver, ſome beef cut in very ſmall pieces; I covered the bottom with it thinly, and poured upon it water, about two inches high. The 22d, the putrid ſmell was very ſenſible: but I let it ſtand till the 24th, when I poured off the fluid [*d*], adding again about the ſame quantity of water to the fleſh. I filtrated then the fluid through a piece of fine linen, and mixed with ſome of it the ſyrup of violets, which it did not alter; neither did it efferveſe with the ſpirit of vitriol, diluted to a ſharpneſs near that of the vegetable acid. I thought of keeping it in digeſtion for ſome days; but, for fear that ſome little ſolid particles might have paſſed through the linen, and by that means, in growing putrid, might give ſome alcali, and render the trial inaccurate, I diſtilled the fluid by a heat of about 160°, after which, I repeated the trial with the ſyrup of violets and the ſpirit of vitriol; but it produced no change.

[*b*] Acta Taurinenſ. Vol. I. p. 78. Cum attegerint ſummum efferveſcentiae gradum, continuato ejuſdem loci calore efferveſcentiae vim amiſerunt. P. 79. Citius plerumque prodiit foetor, quam alkali, idemque tardius deſiit. P. 82. Maſſam inde relinqui foetentiſſimam, ſed emiſſo alkali ad efferveſcentiam ineptam.

[*c*] Id. p. 83, 15. Quum foeteret graviſſime reſiduum deſtillationis, quamquam omni alkali orbatum, manifeſtum videtur, ab alkali foetorem exaltari quidem poſſe, & magis penetrantem effici, non autem ab eodem produci, quandoquidem ſupereſt eo ſublato—16. Videtur is odor a volatilibus admodum particulis proficiſci, ſed quae ab alkali diſſimiles ſunt, plerumque citius gignantur, tardiuſque diſſipentur—alcaleſcentia adeſſe poteſt medico foetori conjuncta—viciſſim maximus foetor abſque alcali—Ex quibus differentia inter foetidas alcalinaſque partes confirmari videtur.—P.84, 17. Videtur alcali non eſſe productum neceſſarium putrefactionis neque gradum alcaleſcentiae gradui putrefactionis reſpondere.

[*d*] It requires ſome attention to find out the proper time when to pour off the liquor; if it is done too ſoon, it will give too little volatile alcali to be much ſenſible by experiments; for, though it ſmells ſtrongly, it is known how little matter is required to produce a ſtrong ſmell. If it is delayed too long, it ſhews already ſigns of an alcali. For that reaſon, I made many experiments in vain.

change. I then put it, the 25th, into a retort, fitted to it a receiver, applied to the jointure a ring of paſte made of flower and water, covered it with a piece of wet bladder, and expoſed it in a *balneum arenæ* to a heat of 108° to 116°, till the 29th of June, when the whole fluid was diſtilled over. I perceived during this operation, that the liquor, from being quite tranſparent, grew turbid; the firſt diſtilled tranſparent fluid grew alſo turbid in the receiver, and at the bottom of the retort there was a ſmall ſettlement of a whitiſh earth. The liquor had a particular ſmell, but quite different from a putrid one, inclining to the volatile alcali; and ſhewed a ſlight but ſenſible degree of effervеſcence with the ſpirit of vitriol; and the ſyrup of violets was turned evidently green by it.

In the mean time, the fleſh with the water continued to emit a putrid ſtench; and the 28th of June I found the fluid colouring the ſyrup of violets greeniſh, and ſhewing a kind of effervеſcence with the acid. Both theſe qualities were increaſing every day, till the 8th of July, when, on account of a journey, I could not obſerve it any longer. I had left the mouth of the receiver open; and on my return the 1ſt of Auguſt, I found an exceeding putrid ſmell; I covered the veſſel; and the 2d, examined the fluid, but it did not effervеſce any more. I then filtrated the liquor; but the fleſh was ſo rotten, that a great many particles paſſed through the linen, and rendered it turbid. I put it into a retort, adapted a receiver, and luted it, as before-mentioned; the heat was alſo the ſame, between 108° and 116°. In this warmth it continued for about four days, when the fluid was diſtilled over. On opening the veſſels, the ſmell was again entirely changed, not near ſo diſagreable as before. In the receiver I obtained a fluid, which turned the ſyrup of violets green, effervefced very ſmartly with the very ſame ſpirit of vitriol I had uſed before; gave the ſmell of a volatile alcali, on adding to this the fixed alcali; praecipitated the calces of metals diſſolved in acids, and ſhewed itſelf by all proofs a true volatile alcali. In the retort remained a yellowiſh matter, almoſt without any ſmell. I put to it ſome water; and after 24 hours, it gave the herbaceous ſmell, but ſhewed no ſigns of any alcali. I let it ſtand four days longer: the herbaceous ſmell continued; but there was no alcali to be diſcovered. I diſtilled it with a gentle fire: but neither then did there appear an alcali [*e*]; and by applying a ſtronger fire, I got nothing but a kind of empyreumatic oil.

I had poured, the 3d of Auguſt, ſome freſh water on the putrid matter; its putrid ſmell continued; the 7th I decanted the fluid, filtrated it, and made it undergo the ſame operation, with exactly the ſame effect as before; which I did again the 11th, with the very ſame effect. I did not repeat it oftener, as I had occaſion for this putrid fleſh to ſome other purpoſe.

Theſe experiments ſhew, I think, that the volatile alcali is preſent as long, at leaſt, as the putrid ſmell continues; and that this volatile alcali is the baſis of it, becauſe, as this was diſtilled over, the reſiduum, being ſtill in inteſtine motion, got only the herbaceous ſmell. The reaſon, why the volatile alcali has been diſtinctly obſerved at a certain period of putrefaction, and not in the others, is, I believe, this; the volatile alcali has, it ſeems, a tendency, to diſintangle itſelf, by inteſtine motion, of all ſuch matter as it is involved with; but if it is not combined with ſuch fixed matter as retains it till it has gone through all its evolutions, it is, being itſelf volatile, carried off by the ſtill more volatile phlogiſtic matter with which it is commonly joined. For this reaſon, I ſuppoſe, the putrefying matter ſhews in its beginning no ſign of a volatile alcali; becauſe its ſmell depends only on thoſe particles, which have been on the ſurface, without any ſtrong coheſion with the ſubſtance. In the farther progreſs of putrefaction, the matter involving the alcali, or forming it, is intermixed, and in coheſion with the ſolid particles of the ſubſtance, and is by theſe means retained till the alcali is come to its purer ſtate. Towards the end of putrefaction, the coheſion of the particles being almoſt entirely taken off, the volatile alcali is carried off before it can go through all its ſtates.

[*e*] What this herbaceous ſmell did depend on, I did not enquire any farther, as not relating to medicine, ſince a living body never was found in ſuch a ſtate: but very likely it depends on ſome volatile alcali, which is perhaps in ſo very ſmall a quantity as not to be perceptible by experiments.

If it is therefore true, that the volatile alcali is eſſential to, or at leaſt always preſent in putrefaction, it ſeems to follow, that the alcalies never can be uſed in living bodies, as antiſeptics [*f*], for laying aſide their ſtimulating quality, which muſt prevent their uſe in moſt of the putrid diſeaſes, they would increaſe the morbific matter, by being intimately mixed by circulation with phlogiſtic matter, which they find in abundance in ſuch bodies. It has been objected to this, that the exhalation of ſtale urine, though ſhewing a great quantity of volatile alcali, is inoffenſive to health [g]: and that ſome perſons have taken the volatile alcali in very great quantity, without its bringing on a putrid diſeaſe [h]: but there

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[*f*] It is very difficult, methinks, to account for the antiſeptic power of the volatile alcali, and other ſalts, on dead animal ſub-

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ſtances: I once thought, that as the ammoniac ſalt, nitre, &c. bring down the thermometer ſeveral degrees, perhaps all theſe ſalts acted by inſtantly abſorbing the heat produced by the beginning inteſtine motion; and that, as a certain degree of warmth is neceſſary to putrefaction, in preventing this degree from coming on, it might hinder the whole operation. To ſee by experiment how far this might be true, I put into phials a certain quantity of water, with that proportionate quantity of alcalies, fixed and volatile, ſal ammoniac, &c. which Sir John Pringle had found (Append. p. xvi. xvii.) to be antiſeptic; and in one as much pure water as a ſtandard. I ſtopped every one of them with a cork, in which I had made a hole for a thermometer of Fahrenheit. I expoſed all theſe phials to the ſame heat; Sir John had uſed about 112°; but I found, that both thoſe with the ſalts and that without it marked the ſame degree of heat; and that therefore the abſorption of heat can by no means be the reaſon of the putrefaction being ſtopped. May this phænomenon not depend upon the ſalts penetrating the body, and giving to the particles more *puncta contactus* (according to their greater or leſs affinity)? and may not theſe ſalts, in augmenting coheſion, hinder the fluids from ſeparating themſelves from one another, and, in conſequence, prevent inteſtine motion? Is this not ſomewhat confirmed by the action of adſtringents? and by the moſt powerful actions of metallic ſalts, as being of the greateſt ſpecific gravity?

[g] Sir J. Pringle, Append. p. vi.

[h] Id. ibid. p. xcii.

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are however ſome examples[i], where it has been hurtful. It is urged further, that a perſon, being only for a ſhort time expoſed to really putrid exhalations, may be infected with putrid diſeaſes; and therefore that this effect of putrid exhalations does not depend on the volatile alcali, as it may be taken pure in very large doſes, without producing ſuch effects. To this I reply, by an analogous inſtance; a ſmall quantity of ferment will bring on fermentation in a large maſs of fermentable matter, and yet as much acid as could be obtained from the ferment, far from exciting an inteſtine motion in the fermentable matter, would rather check it; but can it, for all that, be denied, that the involved acid in the ferment is the chief cauſe of ſetting the whole maſs in fermentation? In the ſame way, the alcali combined with phlogiſtic matter may produce ſuch inteſtine motion as the pure alcali cannot; and very likely the firſt would not produce it, if the volatile alcali in it could be changed.

To bring this about, the moſt powerful means ſeem to be the uſe of acids; and the moſt celebrated phyſicians agree in the good effect they have obſerved from acids in putrid diſeaſes, and recommend them ſtrongly. Dr. M'Bride thinks otherwiſe, and his reaſons are theſe: *firſt* that if the acids came unchanged to the abſorbent veſſels, they would not admit of them [k]; *ſecondly*,

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[i] Huxham on the ſore throat, p. 67, 68. Ejuſd. Eſſay on fevers, p. 118, edit. 5.

[k] Experiment. Eſſays, edit. ſec. p. 20. The auſtere acid (generated in the firſt paſſages of weakly perſons) is exactly in the ſame ſtate with a foreign acid, for the lacteals will admit none of it.

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if they did, they would be dangerous [l]; and *thirdly* that they are quite changed, before they leave the *primæ viæ* [m]. As for the *firſt*, I do not know what reaſons Dr. M'Bride founds his aſſertions upon, as acids never are given in ſo concentrick a ſtate, as by their aſtringency to make theſe veſſels ſhut up their orifice; and as metallic ſalts themſelves are abſorbed in their very compound ſtate (which ſeems clear with regard to the corroſive ſublimate, and other ſuch ſaline preparations), I do not ſee, why the ſimple acids could not be abſorbed. The *ſecond* reaſon ſeems to be founded upon ſome of Dr. M'Bride's experiments (p. 132, 133), *viz.* that putrid fleſh, ſweetened by diſtilled vinegar and ſpirit of vitriol, was firm; but on being boiled went quite to pieces, whereas that ſweetened by volatile alcali did not. But, I conceive, theſe experiments are not applicable to a living body: for the acid being there mixed with the fluids, cannot act in this way on the ſolids, till the fluids are (if I may uſe that expreſſion) ſupra-ſaturated with the acid [n], which in putrid diſeaſes cannot be the caſe. And farther, a heat of 212° of Fahrenheit never can increaſe the action of the acids in living bodies, as it did in the experiments; for, though Dr. M'Bride denies this conſequence, and will prove the contrary, as the fleſh with the alcali did not diſſolve; yet this circumſtance proves nothing more, than that the volatile alcali has not ſuch power of diſſolving the gluten of animal fibres, as acids have; for, if the effect depended only on the action of the acids by themſelves, the fleſh would rather have been diſſolved when immerſed in them, than when boiled in water.—The Doctor beſides ſeems not quite conſiſtent on this head; for, p. 151, he ſays, "Adſtringents can only "be of importance in thoſe caſes, where, from "extreme relaxation and reſolution of the ſolids, the "diſſolved fluids are ſuffered to tranſude, and either "form ſpots of different hues, or run off by actual "hæmorrhage; here, indeed, the acid of vitriol, as "an aſtringent, not as an acid, is found of great uſe "in gaining time." As the acid could not exert its aſtringent power on the veſſels, without coming to the *ſecundæ viæ* (p. 153.) he ſeems not afraid, in this caſe, of its melting down the moſt ſolid parts to a jelly.

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[l] Ibid. p. 134. the acids diſſolve the elementary earth, and thus deſtroy the texture of that ſubſtance, whoſe ſoundneſs they are ſuppoſed to reſtore.—P. 148. we are not to expect, that they are to pervade the minute branches of the vaſcular ſyſtem; when indeed it is evident, that they ought not to be allowed to paſs into the blood in their acid form; ſince it is plain, that, from their diſſolvent nature, the body muſt be deſtroyed, and its moſt ſolid parts melted down to a jelly, if naked acids were to be received into the general maſs of fluids.

[m] Ibid. p. 148. acids are neutralized during the alimentary fermentation; and therefore they cannot act as acids, by faturating any thing of the alcaline kind that they meet with in their courſe of circulation.

In proof of his *third reaſon*, he alledges ſome experiments; *viz.* the third, p. 40, where a mixture of *fleſh*, bread, lemon juice, and ſaliva, did not effervеſce, after fermentation with an alcali; and the 5th,

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[n] This has, it ſeems, happened in ſome rare caſes quoted by Dr. M'Bride, and Dr. Haller, p. 148.

p. 42, where a mixture of bread, water, ſaliva, and ſpirit of vitriol effervеſced ſmartly, before the inteſtine motion; but not at all after it. I could object againſt theſe experiments, and eſpecially the 5th, that perhaps the proportion of the ſaliva to the acid was too great, and that a perſon in a putrid diſeaſe ought to take more acids than could be neutralized by the inquiline liquors. However, I will not inſiſt on this; and ſuppoſe theſe experiments to be quite applicable to the caſe: but if theſe mixtures do not effervеſce any more, does it follow, "that "they are neutralized, and therefore act as acids, by "ſaturating any thing of the alcalinous kind, that "they meet with, in their courſe of circulation?" There are ſome ſaline bodies, which do not effervеſce when mixed together; which will, however, change one another's nature. Thus *ſ. e.* brimſtone, mixed with a ſtrong fixed alcali, does not effervеſce [*o*], but changes, on being diſſolved, the nature of the alcali. A ſolution of ſoap does not effervеſe on the addition of an acid, but joins with the acid, and neutralizes it. Theſe inſtances made me ſuſpect the concluſion drawn by Dr. M'Bride from his experiments; and to clear up theſe doubts, in this particular caſe, I referred to experiments. For this purpoſe, I mixed, the 4th of Auguſt, the thermometer being at 64°, three ounces of ſaliva, a dram of the liquor of putrid fleſh, and a very ſmall quantity of bread: and added as much of the diluted ſpirit of vitriol, as to make it ſour, and efferveſce definitely with the alcali. There was not any ſign of inteſtine motion till the 7th of Auguſt, when from time to time ſome air bubbles, and alſo ſome ſolid particles, roſe to the top; and this continued till the 8th. Not perceiving any farther motion, I poured off the clear liquor, which did not efferveſce any more with the alcali. I mixed, the 9th, ſix drams of the putrid liquamen, with about the double of this liquor, and put in beſides four ſolid pieces of fleſh, which had lain three days in the liquamen: theſe pieces were of a prodigious ſtench, and ſo rotten, that with the leaſt force they were torn to pieces. There appeared no ſigns of inteſtine motion: the 10th, the putrid ſmell was very much abated: the 11th, it was changed, and there remained only a ſmell much like that of ſound fleſh: the pieces were without any ſmell, and had acquired again ſome degree of firmneſs. In this condition they remained for a week, and I did not obſerve them any longer.

[*o*] This applies alſo to the ſolution of brimſtone in limewater, out of which the lime particles have been precipitated, by the intoduction of fixed air.

This experiment proves, I believe, that acids, though changed in the alimentary canal ſo far, as not to efferveſce with alcalies, may notwithſtanding check putrefaction; and that, therefore, their uſe is of great conſequence, and ought not to be omitted in putrid diſeaſes. Though Dr. M'Bride believes that theſe diſeaſes may be cured with fermentable ſubſtances only; I muſt own that I do not agree with him, and am not quite convinced of his opinion, that putrefaction depends only on the loſs of fixed air. I rather believe this an effect than the cauſe of putrefaction; but I ſhall refer this ſubject to another occaſion.

END OF PART I.